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TWO FUNDAMENTAL PROBLEMS IN PHONEMICS

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[P225. Phonemics--Theory.]

0. This article was originally planned as a review of several recent works of Kenneth L. Pike.[1] Although it has grown somewhat beyond the bounds of a review, the writer has found it useful to retain the procedure of using Pike as an advocatus diaboli--not, it is hoped, as a straw man.

Pike and his coworkers are engaged in the difficult task of making linguistic field workers out of large numbers of people--who come to them with no previous linguistic training and often with little apparent linguistic ability. In order to get anywhere near their goal, the Pike staff have had to prepare their own textbooks: teaching materials in which the whole task of analyzing a language is broken down into the smallest possible separate steps, for individual discussion and practice. This, of course, is a good idea for any beginner. The Pike-trained student will face a practical job: he must learn an 'obscure' language, analyze its structure, devise a practical orthography, conduct literacy campaigns, and, above all, translate highly refractory material into the language. In training such students, Pike and his co-workers must keep these practical considerations constantly in mind. As a result, many definitions and concepts assume a guise somewhat different from what would result in a purely scientific context.

Of this we shall give two preliminary examples. By 'phonemics' Pike apparently means just what he says in the subtitle of his book on that subject (Pike, 1947a): a set of operations that can be performed with the end product of a practical orthography (save, at most, for the choice of graphic shapes with which to represent whatever it is one represents). The scientific approach, as the writer sees it, would keep the following two matters separate:

(1) the task of determining the phonemic structure of a language, and (2) the task of devising a practical orthography for use by the speakers of the language. True enough, the second involves the first; and the first involves something related to the second, in the sense that we have to develop notations with which to handle our material at every stage.[2] In one way these alternatives are perhaps only arbitrary preferences for one or another use of a term, but behind this lurk some real differences of method to be discussed presently.

Again: from the scientific point of view, the expression 'tone languages' (Pike, 1948—title of book) may be no more meaningful than, say, 'velar spirant languages' or 'triangular five-vowel-system languages'. Such terms may form no part of an objective linguistic taxonomy. And again the justification for the expression is practical. Pike's students are speakers of English, for whom the various phenomena subsumed roughly under the rubric 'tone' constitute a special kind of difficulty.

An elementary textbook should not be expected to constitute a contribution to scientific knowledge. In chemistry, such a textbook is built on a broad base of accepted doctrine, and a reviewer passes judgment only on the effectiveness of the pedagogical devices used. Linguistics does not yet stand with chemistry; in writing an elementary textbook one has to choose in some cases between different and incompatible fundamental theories. A reviewer can still discuss pedagogical merits, but such discussion is sometimes only of secondary importance. It would be in the case of Pike's recent books. Here we shall discuss only those two points in Pike's theoretical frame of reference which seem weakest: the avowed use of 'grammatical' criteria in phonemic analysis, and the notion of 'potentials'.[3]

1. Pike's volume Phonemics makes use of 'little-language' problems, sometimes coined quite a priori, sometimes derived by restriction from some actual language. The contrast between Pike's point of view and that which we are going

to defend here can best be shown by starting with one of his problems. We choose problem 208 (Pike, 1947a, p. 159—further page references are to this work). The raw data are:

[op] 'horse'	[basop] 'the big horse'
[bas] 'big'	[opzuMbas] 'the horse is big'
[zuM] 'is'	[opmazuMbas] 'his horse is big'.
[ma] 'his'	

We are to assume either that these seven utterances are the whole language, or else that any conclusions we base on these data will not be controverted by further data; and we are to assume that the phonetic transcription is correct and complete. Pike's instructions to the student are '1. Rewrite phonetically the last three utterances, adding spaces in the appropriate places. 2. Rewrite the same data phonemically.' His solutions are, phonetically [bas op], [op zuM bas], [op ma zuM bas]; phonemically /baz ob/, /ob zum baz/, and /ob ma zum baz/.

In the preceding discussion he says (159): 'It is frequently convenient to write spaces between items which themselves may be found elsewhere as constituting complete utterances.' Later: 'Any morpheme or close-knit sequence of morphemes which can occur at the beginning, end and middle of utterances should be preceded and followed by a space.' That is, we are to determine where grammatical boundaries of certain types fall, write them with spaces, and then regard these spaces as parts of environments in determining distribution of sounds and achieving our final notation. This is clearly what he means; yet in the first quoted passage there is no explicit reference to grammatical forms and borders—he uses the colorless term 'item', which might be purely phonological. We shall show presently that if 'item' is so interpreted, then the passage shows us how to introduce purely phonological junctures, without ringing in any grammatical considerations.

If we ignore Pike's instructions for problem 208, and simply examine distributions, we observe that [b] and [p], and likewise [m] and [M], are in complementary

distribution, since the voiceless member of each pair occurs only before another consonant or at end of utterance. We could therefore replace the paired symbols by single symbols, say /b/ and /m/, and have an entirely satisfactory phonemic notation, in which the seven utterances would appear as /ob/, /bas/, /zum/, /ma/, /basob/, /obzumbas/, and /obmazumbas/. Note that it is still necessary to keep [z] and [s] separate, since they contrast intervocalically. Now it is unlikely that Pike intended the problem to be so simple. He probably meant for the contrasts between [b] and [p], [m] and [M], to appear distinctive until the introduction of spaces. In all fairness, therefore, let us modify his data in such a way as to make this so. Let us add to the raw data the utterances [ba] 'not', [za] 'cow', [opop] 'many horses', [mazuMop] 'his is a horse', [opzuMbabas] 'the horse isn't big', and [zama] 'his cow'. The enlarged corpus appears, without meanings, in column I of the Table. In this new assemblage, [b] and [p] contrast between vowels, as do [m] and [M], but otherwise there are no essential changes of apparent structure.

If we follow Pike's instructions—first inserting spaces between 'free forms', and then 'phonemicizing' in terms of the modified environments in which our symbols appear—we obtain the solution given in column IV of the Table.

If again we ignore Pike's instructions, then we must conclude that the enlarged corpus appears already, as raw data, in a notation which by the reviewer's

TABLE

Utterance	I	II	III	IV
(1)	[op]	/ob/	/op/	/ob/
(2)	[bas]	/baz/	/pas/	/baz/
(3)	[zuM]	/zum/	/suM/	/zum/
(4)	[ma]	/ma/	/Ma/	/ma/
(5)	[ba]	/ba/	/pa/	/ba/
(6)	[za]	/za/	/sa/	/za/
(7)	[basop]	/baz ob/	/pasop/	/baz ob/
(8)	[opzuMbas]	/ob zum baz/	/op suM pas/	/ob zum baz/
(9)	[opmazuMbas]	/ob mazum baz/	/op Ma suM pas/	/ob ma zum baz/
(10)	[opop]	/ob ob/	/opop/	/ob ob/
(11)	[mazuMop]	/mazum ob/	/Ma suMop/	/ma zum ob/
(12)	[opzuMbabas]	/ob zum babaz/	/op suM pa pas/	/ob zum ba bas/
(13)	[zama]	/zama/	/sa Ma/	/za ma/

definition of the term must be considered phonemic. That is, the notation indicates in each position all the contrasts which are distinctive there, and no others; it indicates each such contrasting element with a single and consistent symbolization. However, the notation of Column I is only one of a great many possible phonemic notations in this sense, and we may profitably discuss what other purely phonemic notations are also possible. We shall derive three others, each no more and no less phonemic than that of column I.

(1) First we take advantage of the fact that in utterance-final only [s p M] occur, to the exclusion of the paired voiced sounds [z b m]. We break [s p M] into two simultaneous components each, respectively /z b m/ and unvoicing. We then agree to use no special symbol for devoicing in utterance-final, since it is automatic there; medially, we will write it with a space placed directly after the symbol representing the affected consonant.

The utterances then appear as given in Column II. When columns II and IV are compared, it will be noted that without bringing in any grammatical criteria we have introduced spaces at nine of the thirteen points where Pike's procedure would introduce them, and nowhere else; the four exceptions are in utterances (9), (11), (12), and (13).

(2) Next (starting over again), we take advantage of the fact that in utterance-initial only [z b m] occur, to the exclusion of the paired voiceless sounds. We break [z b m] into two simultaneous components each, respectively /s p M/ and voicing. We then agree to use no special symbol for voicing in utterance-initial, since it is automatic there; medially, we will write it with a space placed directly before the symbol representing the consonant affected. In this notation, the utterances appear as given in column III. Comparing columns III and IV, we note that there is a one-to-one correspondence between the /s p M/ of III and the /z b m/ of IV, so that the difference is merely one of shape of individual symbol, and quite irrelevant; and beyond this that without bringing in any grammatical criteria we have once again introduced spaces in most of the places in which the Pike solution introduces them (ten of his thirteen places), but nowhere else; the three exceptions are in utterances (7), (10), and (11).

(3) Finally, we go a step further, and extract from the pairs [z : s, b : p, m : M] both voicing and unvoicing. What is left in this case is a concatenation of articulatory features that can only occur in a simultaneous cluster with one or the other of our two components; this need disturb us no more than that most 'consonants' in most languages can occur only in sequential clusters with 'vowels'. Choosing the symbols /z b m/ for these abstracted concatenations of features--purely so that the results will look more like Pike's--we define them respectively as apico-dental groove position (?--the phonetic details are of course not supplied), bilabial closure with velic closed, and bilabial closure with

velic open. Since initially and finally in utterances the presence, respectively, of voicing and of unvoicing is automatic, we agree not to use any extra symbol in these positions for either feature. Medially, we agree that voicing will be written by a space before the symbol representing the consonantal articulation affected, unvoicing by a space after the symbol; and we agree that where these conventions would lead us to write two spaces in succession, we will write only one--no ambiguity can result. The utterances appear in this notation in column IV--that is, precisely in the shape that Pike would give them. We have thus achieved an identical symbolization without bringing any grammatical considerations to play.

The reader at this point may be inclined to ask: Why go all around Robin Hood's barn if it gets us just where Pike gets us more directly? Our route, it is true, is a seemingly lengthy one, and his apparently direct; but ours lies entirely on firm ground, whereas the Pike 'shortcut' may turn out to lead across a bog in which we can lose our way irretrievably. I cannot resist an analogy: Most people would agree with Euclid that it is perfectly clear intuitively which is the 'inside' of a triangle, which the 'outside'; but unless one provides formally for a definition of 'inside' and 'outside', it is possible to prove Euclidean plane-geometric propositions which are patently not the case (Lieber and Lieber, 1944). Now, in linguistic analysis, suppose we are confronted with the following set of forms:

[ga] 'this'	[gali] 'that'
[lima] 'boy'	[ma] 'stone'
[galima] 'this boy'	[galima] 'that stone'

Following Pike's procedure, we would first insert spaces to get [ga lima] and [gali ma] respectively. But it is obvious that the phonetic sequence [galima] is identical regardless of whether it means 'this boy' or 'that stone'. Introduction of border markers on the basis of grammatical considerations, it would

seem, can conceal identities of environment and thus lead analysis astray. This does not mean that we might not insert the spaces in these last forms; it does mean that we would find no phonemic reason for inserting them, and consequently would be putting them in at some later point in the whole operation of developing a 'practical orthography', not during the process of phonemicization. In other words, there are languages where there are phonological data susceptible of a kind of interpretation that would lead us to write spaces, and other languages where there are not.

This brings us to a consideration of the two different kinds of 'word' and 'morpheme' current today. Pike seems to accept a definition of word involving meaning: a minimum free form which occurs as a whole utterance with approximately the same meaning it has when in included position. But it is also possible to define a phonological word: any stretch of phonemes which occurs as a whole utterance, and which cannot be broken into two or more shorter stretches which also so occur, quite regardless of meanings. If in the modified problem 208 [basop] meant 'bishop' instead of 'the big horse', then Pike would presumably not choose to retranscribe it as [bas op] or /baz ob/, since /baz/ and /ob/ do not occur (according to the corpus) with any meanings such that the sum of the meanings could be 'bishop'. But our procedure would apply exactly as before, and with the same results. Our phonological inspection has revealed the structuring of the longer utterances of the corpus in terms of sequences of 'phonological words', has revealed that the borders between phonological words in longer utterances are phonetically marked, and has made the most of that fact to develop a notation which uses symbolisms in a mnemonically convenient way. In the case of [galima], our procedure would show that this is not a phonological word, since there exists shorter phonological words such that [galima] can be taken as a sequence of two of them in either of two ways; it would show, however, that there is not any phonetic marking of the borders between phonological words in included position,

so that on the phonological level there would be no justification for any retranscription with spaces or other border markers.

I should not hesitate to call the complex of phenomena symbolized by spaces in our third rephonemicization of the modified problem 208 a phoneme; specifically, a junctural phoneme or simply a juncture.[4] It is, so to speak, an utterance-medial event parallel to what happens between the end of one utterance (providing it ends with a consonant) and the beginning of the next (providing it begins with a consonant), with the sole difference that in the latter situation there is a pause, lasting anywhere from a second to a day or week, whereas in the former there is no pause. We can define our juncture in this problem as transition from voicelessness to voicing, when it occurs flanked by consonants; if no consonant precedes, then the juncture has, as another allophone, inception of voicing without any preceding interruption of voicing; if no consonant follows, similarly; the juncture of course does not occur with neither preceding nor following consonant. Indeed, this is a good simple example of what I should mean by 'junctural phoneme': If some particular phenomenon is predictable at the ends of utterances, either always or whenever certain other features are found there, but is not predictable medially, and if its successive occurrences seem to mark fairly well the borders between phonological words, then that phenomenon is junctural; similarly for such a phenomenon predictable initially but not medially. (In many languages--perhaps in most actual languages, in contrast to the simplified languagettes of Pike's book--some modification of the above definition is needed, along the line of relaxing the requirements to be met by a stretch of phonemes for it to be considered free.[5] Thus in English we will get one set of junctural phenomena with the definition as originally given; if we then 'lift off' intonation phonemes and define phonological words relative to the abstracted remainder, we get another set; if, then, we also abstract from all stress differences except that between no stress at all and a non-zero stress level, and define phonological words relative

to the remainder, we get still a third set.) A juncture phoneme is then a grouping of such phenomena which makes for unambiguous and simple linear transcription. If this last statement seems arbitrary, it is no more so than is, for me, the definition of any kind of phoneme. One can derive, from the totality of distinctive features in the utterances of a language and the arrangements in which they occur, various different sets of bundles to be represented by unit symbols in a linear transcription, and any such derivation establishes what I should call a phonemicization. A phoneme is a phoneme only as part of a stated phonemicization. The 'phonemic structure' of a language is not to be equated to any such phonemicization; rather it is the 'least common denominator' of every possible sufficient phonemicization:[6] that which must be subsumed by any set of phonemic symbols with accompanying instructions for use. We have already demonstrated four different phonemicizations of modified problem 208, which will illustrate the point just made. It also follows that juncture is never forced upon us; it is always possible to phonemicize without junctures, though perhaps only sometimes possible to phonemicize in a way that gives us juncture phonemes.

Juncture phonemes, as set up on the above basis, may indeed have one property which is superficially different from the properties usually ascribed to phonemes. We do not ordinarily classify a group of allophones into a single phoneme unless all the allophones have some configuration of phonetic features in common, however much they may differ as to other phonetic features. But the various allophones grouped into a single juncture phoneme may have nothing in common. Two of the allophones of the juncture phoneme introduced into the third solution of modified problem 208 have, it would seem, no feature in common: i.e., the allophone consisting of momentary cessation of voicing on a consonant without resumption of voicing on a second consonant, and the allophone consisting of voicing on a consonant without momentary cessation of voicing on a preceding consonant. Now, mathematically, a class is considered to be defined when, if presented with any

object, we can say whether or not that object is a member of the class. One way to set up a class is to specify a property, so that possession of that property by an object puts it in the class, lack of that property leaves it out of the class. Thus 'even integer' is a property which establishes a class including 2, 4, 6, etc., but not 1, 3, 5, etc., nor a leaf nor a nose. But another perfectly satisfactory way to define a class is by itemization. Thus I can establish a class consisting of the first three even integers, all the leaves on the tree outside my window, and the third word on this page. A class so defined is a perfectly good class; it even has properties that can be investigated, e.g. it consists of a certain number of objects. If we insist on tying up a class with a property, in the case of a class of this kind we can regard the property of membership in the class enumerated as the associated property. That is, a class can be defined in terms of a property, or vice versa. So we see that the failure of all the allophones of some juncture phonemes to have some (articulatory or acoustic) property in common is no logical defect. We may with perfect validity define a juncture phoneme as consisting of all occurrences in certain specified contexts of certain specified phenomena. It may well be doubted whether we would wish to extend this type of class-definition to other phonemes, although perhaps we do in actual practice: in certain dialects of English we class [ʔ] into a /t/ phoneme, though in doing so we may have to admit that the only 'property' all our /t/ allophones share is 'non-[k]-ness' and 'non-[p]-ness'. Juncture phonemes, where recognized, do consist of allophones with a structural property in common; ultimately this may actually be more important.

In the above lengthy defense of juncture I have not intended, of course, to claim that all juncture 'phonemes' or 'junctures' set up so far for various languages, or even all set up in previous discussions by myself, would really pass the tests here established. Where they would not, however, I should not feel that the analysis is defective.

There are two different approaches to morphemes just as there are two different kinds of words. Pike accepts a semantic definition: 'A MORPHEME is the smallest meaningful unit of linguistic structure, such as boy and -ish in boyish' (60, right-hand column, fn. 2). But there also exists a more formal definition, of the Harris type: a morpheme is an arrangement of phonemes such that the occurrence of phonemes in utterances (long as well as short) is most efficiently stated in terms of the phonemic structure of morphemes and the morphemic structure of utterances, rather than directly in terms of the phonemes themselves.[7] True enough, even with morphemes defined this way, we often find that meanings seem to be more or less regularly attached to them, and we can use meanings as hints to tell us what the morphemes of a language may be, without thereby abandoning the fundamental purely formal definition. And when morphemes are defined formally, it becomes perfectly obvious that use of position in morphemes or relative to morpheme borders as a criterion in phonemic analysis is completely circular.

The formal definition of morpheme also brings out the importance of the phonological word, and, where possible, of junctures defined in terms of phonological words. From the very definition of phonological word, such sequences of phonemes are going to be maximally useful units in stating the structures of whole utterances. Phonological words (with or without phonetically marked boundaries) are larger units capable, by definition, of the greatest amount of reshuffling from utterance to utterance. There are of course limits to this shuffling--otherwise 'syntax' would be easy--but the degree of freedom is none the less great. They are, in fact, the 'items' to which we wish Pike were referring in the passage already quoted ('It is frequently convenient to write spaces between items which themselves may be found elsewhere as constituting complete utterances.').

I should say that it is not yet certain whether formally defined morphemes are actually what concern us more, rather than semantically defined morphemes; perhaps the two are simply different units, with some correlation between them

but without identity. The formal morpheme needs much further investigation. But even with the semantic definition, it would seem clear to me from the above considerations that use of morpheme borders in phonemic analysis is unproductive. To take up again the 'short cut' analogy, we will do better to regard Pike's 'short cut' through grammatical territory not as a shorter route to the same goal at all, but rather as a direction in which we can look (without venturing to proceed for fear of the bog) before taking the long but safe road around, and from time to time while en route.

2. Now so far in our discussion we have been accepting one of Pike's procedural premises and framing our argument within it. That is, we have agreed to handle small corpora of data on the assumptions he gives: either that such a corpus constitutes the whole language, or that nothing found elsewhere in the language will ever controvert conclusions based on the small corpus; and, in either case, that the data given are phonetically accurate and complete. Except for one factor, this is a reasonable enough assumption. Given time enough, a field worker can arrive at a transcription which has the characteristics of Pike's problem corpora, although the experienced field worker is apt to reach a tentative phonemicization long before he observes and provides for all the phonetic details that turn out to be relevant (Voegelin, 1949). There is, however, one factor in the small corpora which tends to invalidate the approach unless great care is taken: their size. If we accept one of the alternative assumptions, namely that the small corpus is the whole language, then we are obviously unrealistic. If we accept the other alternative, that nothing else we may later add to the corpus will contradict any conclusions based on it, it is possible to demonstrate that this assumption actually precludes there being any other utterances in the language, and we are back again with the first assumption. For from the data of modified problem 208, we could quite validly conclude that [b] occurs only before [a], that [o] occurs only before [p], and so on; in fact, that [b] occurs initially only when the

remainder of the utterance is [as], [a], or [asop]. The brighter beginning student is often very much inclined to reach such conclusions; we have to muddle up our instructions in order to prevent it. The unrealism of such conclusions does not lie in the logic; it lies in the smallness of the corpus and in the accompanying assumption. Actually, no corpus is ever complete. We work by a method of successive approximations. We state what seems to be the case in terms of the material gathered up to a given time, involving intuitions gathered from experience with other languages; when the material gathered is sufficiently extensive, we can venture to predict with some hope of verification that certain additional things will in time be observed, certain other additional things will not. Only by a recurrent cycle of prediction, checking, gathering of new data, modification of predictions, and rechecking, can we eventually arrive at a description which is accurate to within any prescribed amount of tolerance.

In this sequence of operations, the fundamental assumption of the field worker is Bloomfield's fundamental assumption of descriptive linguistics: In any speech community (and a fortiori for any single speaker) some utterances are alike. 'Utterance' here means 'utterance-event', a unique historical event, at a given time and place. Bloomfield's postulate is an existential assumption rather than an operational one; it is the shape taken in linguistics of a general principle of science recently formulated (Wiener, 1948, p. 62-3) as follows: 'For the existence of any science, it is necessary that there exist phenomena which do not stand isolated. In a world ruled by a succession of miracles performed by an irrational God subject to sudden whims, we should be forced to wait each new catastrophe in a state of perplexed passiveness. We have a picture of such a world in the croquet game in Alice in Wonderland; where the mallets are flamingoes; the balls, hedgehogs, which quietly unroll and go about their own business; the hoops, playing-card soldiers, likewise subject to locomotor initiative of their own; and the rules are the decrees of the testy, unpredictable Queen of Hearts.

The essence of an effective rule for a game or a useful law of physics [or any other science] is that it be statable in advance, and that it apply to more than one case. Ideally, it should represent a property of the system discussed which remains the same under the flux of particular circumstances. In the simplest case, it is a property which is invariant to a set of transformations to which the system is subject.'

An existential assumption, such as Bloomfield's version for linguistics of the general principle of science just stated, unfortunately does not tell us how to discover which utterance-events are, as a matter of fact, the same; it merely assures us, as does common sense, that some of them are. Harris only apparently carries us closer when he prescribes that we look for repetitions;[8] that is, we hear someone say something, and perhaps record it, and then ask him to say the same thing again, or even try to say it back to him, thus, if we are lucky, producing a repetition ourselves. In a way, we are really quite uninterested in the individual utterance-event with its date and place of occurrence; and yet that is all we can ever observe. Harris's advice leaves the fundamental problem unsolved, for how can we be sure that the second event is actually a repetition of the first? Imagine such a dialog as this:

Mary: 'I thought I'd go downtown today.'

John (looking up from SIL): 'What's that, dear?'

Mary: 'I said I thought maybe if you could leave the car with me I'd go do some shopping today.'

Mary's second utterance is not a falsehood, and yet the portion after 'said' is not a repetition for our purposes. Suppose she had answered in 'the same words' she used the first time, but with a different speed and intonation. Would her second utterance in this case have been a repetition of the first?

In order to tell, we have to compare different utterances as specific historical events. In Newton's day people worried about the problem of measuring

duration. It was claimed that you couldn't know that the minute from midnight to 12:01 lasted as long as that from 8:00 AM to 8:01, since you couldn't lay one of them alongside the other the way you could compare two objects as to length in space. This problem was solved by common sense and clocks. Two intervals of time are defined as of the same duration if a clock ticks the same number of times during one as during the other. We could use someone's heart as our clock if we wanted to (Haldane, 1946, p. 130); that we do not, but instead use planets and develop intricate and delicate apparatus which will move in a prescribed way with them, is only because the mathematics of our description of the universe is so much simpler this way. The problem of comparing two distinct historic events, be they utterances or other cultural manifestations, is the same. We cannot put one alongside the other. If we have two forms which we think are almost the same, and suspect may be identical, and therefore ask our informant to say one and then the other, we are not comparing any other historical events; we are simply comparing the two we elicit at that moment. Even in this case the informant does not utter two utterances simultaneously; he produces one and then the other. The analyst has to make use of his short-term memory, the type of memory which establishes the 'specious present', to make the comparison. And for events that occur at greater temporal separation, the technique that has to be used is still that of memory.

Memory can be a device within a human nervous system, or it can be some externalized device: written records of all kinds, and electronic recordings, are all external memory devices. When a given utterance-event occurs, we can produce an icon of it with the latter type of apparatus, or a record of it with the former. When, later, we consult our icons and records, the playback in one case, our own speech mechanism in the other, must indeed produce a new historic event in order for there to be any comparison within the specious present with some new or some other old occurrence; but the pattern of sound waves produced by the playback, in

particular, is far more closely similar to the pattern of the original event than is anything we might, early in a field enterprise, reproduce from internal memory; or, at least, the distortion is of a quite different nature. And with any such internal or external memory devices, the most that can be accomplished is the production of more or less accurate images of the original events, in close succession, for comparison within the specious present; our short-term memory is still indispensable.

In general, we find it quite feasible to dispense with any electronic apparatus, and rely on our own nervous systems supplemented by pencil and paper. That this will suffice is guaranteed by the fact that the first part, unsupplemented, is enough for the newly arrived child in a community, who comes eventually to participate in the linguistic behavior of the community on a par with everyone else.

As practical working criteria, as we compare utterances, we use such as the following, in various specific forms: Do the two utterances seem very similar, to our ears, in their phonetic configuration? Do the two seem to occur in somewhat the same context? Do similar stimuli produce them, and do they produce similar responses? As we begin work on a language, we hardly ever put anything down on paper until after we have asked for 'the same' form several times; sometimes we do not actually get 'the same' form, but except in obvious cases we ignore possible deviations and leave corrections for later. The given criteria, as stated, can be summarized as similarity of sound and of meaning. As we start, we can trust neither our ears nor our apperception of the culturally defined meanings of forms. So we proceed by trial and error. Regardless of how much rationalization of analytical procedures we may indulge in, there is an irreducible minimum of language-learning involved in any linguistic field project. An entity cannot function as a linguistic field worker and analyst unless it is endowed with the language-learning apparatus and ability that human beings have, which implies, of course, that for

the present only human beings can be trained as linguists--we cannot yet construct a machine to analyze languages on the basis of the really raw data which comes from the lips of speakers. This is also the fundamental source of disagreement between descriptions. Equally competent observers with comparable points of view can produce descriptions of a language that differ widely, and which, as the work of each proceeds, will only approach complete agreement, without ever reaching it. This is an analog of the fact that two infant arrivals in a speech community will never grow up to speak entirely identical idiolects. That is, the area of indeterminacy is not necessarily in the analyst or the technique; it is in the data. This is the nature of the flux within which we must seek the invariants which we call utterances (not utterance-events), and then, later, distinctive features, phonological words, phonemes, and all the rest.

One more remark must be made here before we turn to Pike's 'potentials'. It has been granted above--nay, proclaimed--that we cannot decide which utterance-events are 'the same' for purposes of further analysis without using meaning (as defined). This use of meaning is (1) differential, rather than specific, in the sense that we use it to tell us whether two utterance-events had more or less similar meanings or not, but are not then concerned (at this stage) with just what those meanings were;[9] and (2) applied to whole utterance-events, not to parts of utterances. Having once concluded which utterance-events should be classed as 'the same' utterance, and having thus assembled a corpus of utterances (not utterance-events) for further analysis, that further analysis can proceed without any further reference to meaning. Our avowed use of meaning here, therefore, does not constitute a concession to Pike's desire to use meaning for the comparison of recurrent parts of utterances. That is one procedure which can be followed in due time; we claim that Pike introduces it at the wrong point in the sequence of analytical observations.

Now it is to be noted that however long an analyst may have been observing a

language, the problem as to whether historical utterance A and historical utterance B are linguistically identical or not--if answerable at all with the memory devices at hand--is open to two and only two answers: 'yes' and 'no'. Heuristically, there is the middle ground of 'I don't know yet, so I'll record the features I think I hear which seem to differentiate them, and suspend judgment until further evidence is in.' But logically there is no middle ground; every such question must in due time be resolved one way or the other. One cannot eat one's cake and have it too. This, however, is precisely what Pike attempts to do with his 'potentials'.

Pike's problem 212 (p. 163) consists of four short utterances and two longer bracketed sequences of symbols:

['tato'lala'lo'tomo'mara] 'I saw Tom and Mary'

['tato'lala'lo/'tomo'mara] (repeated) 'I saw Tom and Mary'.

(The slant line stands for short pause.) His 'phonemic' solution is /tato lala lo tomo mara/ for both. Since all utterances in the corpus begin with onset of stress, and most segments bounded by stress are phonologically free, taking the stress as junctural would be in accord with our definition of juncture. Pike's discussion includes the following remarks: 'Spaces should be written in normal utterances at points where pauses occur, since pauses in natural speech tend to occur only between large grammatical and phonological divisions which are highly pertinent to the language.' And: 'There were alternate pronunciations of the first sentence--one with pause and one without pause. In either case one should normally write a space at points where pauses may optionally be given, that is, at POTENTIAL PAUSE POINTS.'

We have no way of knowing whether or not pauses might be inserted at other points in 'the same' utterance. We are limited again by the corpus size; we should have to conclude that this particular 'single' utterance can be said in either of the two ways transcribed, but not, say, ['tato/'lala'lo'tomo'mara]. Pike's slant-lined notation therefore writes two different things with the same

symbol (space)--point of onset of stress, on the one hand, and point of onset of stress plus possibility of pause on the other. The real problem is: are these two utterance-events actually the same linguistically? Perhaps the first means 'I saw Tom and Mary', the second 'I saw Tom [23 intonation], and Mary.' In this case, we would be led to doubt that the two are actually the 'same' utterance--highly similar utterances, true enough, but not identical. Analytically, (1) we cannot assign phonemic status to something when it is not present, and therefore 'potential pause' cannot be phonemic; (2) we cannot decide both that (actual) pause is phonemic and that it is not. Suppose we used a comma instead of no special symbol, but otherwise accepted Pike's slant-lined notation, giving /tato lala lo, tomo mara/, the comma meaning 'pause here if you want to'. The comma cannot, so defined, represent a phoneme. If we decide that internal pause is not phonemic, then it should not be symbolized. If it is phonemic, then the two utterance-events are not linguistically identical, and cannot be transcribed by a single array of symbols in a purely phonemic notation. We should rather have to define /,/ as actual pause, and transcribe the first utterance without a comma, the second one with one.

To summarize: If contrasts are not considered, at a particular stage in our observation of a language, to be distinctive, then they must not be noted in a phonemic transcription. If contrasts are distinctive, then two utterances which differ in terms of such a contrast cannot be transcribed at the same time in a phonemic notation. There is, in other words, no place for 'potentials' in a phonemic transcription.

Does this mean that there is no place at all for 'potentials'? It does not. It is Pike's preoccupation with the aim of a practical orthography which leads him methodologically astray. No language has yet had a generally used writing system in which a single array of graphic symbols stands unambiguously for a single utterance (i.e., for any member of a class of linguistically identical

utterance-events). Some linguistically relevant features are always left out, so that the single array of graphic symbols stands for two, three, or many more linguistically different utterances; context, and the special conventions of writing, remove any possible ambiguity (except sometimes when they do not).[10] The reader can read aloud the first English sentence on this page with a dozen different intonations, which will prove the point. It is probably desirable for practical orthographies to have this property. In the task of supplying an illiterate speech community with an effective writing system, it is therefore desirable for the technician to give serious thought to which distinctive features in utterances should not be regularly written. At this step in his operations, he rightly takes into consideration such items as Pike subsumes under the rubric 'potentials'.

3. We have gone to some pains to build a thorough foundation for our objections to Pike's phonemic theories. We may now summarize what the differences in point of view seem essentially to be.

It is important to keep clearly in mind the distinction between theoretical frame of reference and actual sequence of operations in the field. A theory which cannot guide practice is idle speculation; a practice not guided by a carefully and logically elaborated theory--itself subject to constant revision in the light of practical experience--cannot be considered part of science. Like the sailing ship which sets a course due west but must tack north and south in order to follow that course, the field worker must know where he is ultimately going, without being inflexible to the impact of the various more or less favorable winds he encounters. When a mathematician is working out a proof of a theorem, he goes where the winds blow him, he follows his 'intuition', he analogizes on the basis of previous mathematical experience; but he has not established the proof and the theorem until it comes out as a series of statements of a specified logical shape. Many an apparent disagreement on method has been resolved when it has become clear that the participants were not talking about the same thing (theory or

practice) at the same time. I have tried to avoid any source of such confusion in the foregoing discussion.

It will be noted that we have not objected to any specific criterion or procedure that Pike proposes as of importance in the total task of achieving a practical orthography. The objections which have been raised here concern the sequence in which the various criteria are applied, the various procedures undertaken. And here I mean sequence in the theoretical frame of reference (in the shape of the mathematician's final proof) rather than in actual field work (the mathematician's search for the proof), though the latter is involved by implication.[11] Accepting the definition of the overall task as the practical needs of Pike's students shape it, it seems clear that Pike's frame of reference is inefficient and misleading, not atomistically, but as a system. The best of his students will learn anyway, as English-speaking children learn to read despite all the confused 'methods' of the educators; the less gifted students will not learn as well as would be possible if they were given theoretically more logical guidance. The really serious danger is that Pike (as a colleague has put it) has over-institutionalized himself, thereby losing the flexibility, the openmindedness towards new evidence and new theory, that any practitioner of our science or any other must constantly maintain.

[January, 1949]

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[1]Those cited below. References to the following works are made in text and footnotes by name of author and year of publication, with page number if needed:

Bernard Bloch, 1948: A set of postulates for phonemic analysis, Language 24.3-46.

J.B.S. Haldane, 1945: A new theory of the past. American Scientist 33.129-45.

Charles F. Hockett, 1947: Review of Nida, Morphology. Language 23.273-85.

Lillian Lieber and Hugh Gray Lieber, 1944: The education of T.C. Mits. New York.

Kenneth L. Pike, 1947a: Phonemics, a technique for reducing languages to writing. Ann Arbor. University of Michigan Publications, Linguistics, volume III.

-----, 1947b: Grammatical prerequisites to phonemic analysis. Word 3.155-72.

-----, 1948: Tone languages. Ann Arbor. University of Michigan Publications, Linguistics, volume IV.

C.F. Voegelin, 1949: Review of Pike, Phonemics. IJAL 15.75-85.

Norbert Wiener, 1948: Cybernetics. New York.

[2]Prof. Wm. G. Moulton, reading an earlier draft of this paper, pointed out that this constant development of notations with which to handle one's data is a part of every science.

[3]Pike's article (1947b) is devoted to the former of these points. I have not dealt piecemeal with his arguments as given there, since most of those arguments lose their weight once their fundamental underpinning is removed--and the latter is the task of the present paper.

[4]This and the following two paragraphs are directed as much at Bloch (in the light of Bloch, 1948, and subsequent discussions) as at Pike. Bloch and Pike seem to stand together in considering juncture a grammatical matter; beyond this, they are poles apart, for while Pike considers this ample basis for bringing in all kinds of grammatical criteria for phonological analysis, Bloch considers it sufficient reason to eliminate juncture from phonological analysis. I am confident that Bloch's postulates could be modified, along lines suggested here, in such a way as to allow for junctures, but can hardly attempt a full demonstration of this here. As to the difficulty Bloch cites in telling how much speech constitutes a single utterance--we could easily enough begin with an arbitrary minimum length of pause which we shall consider sufficient to mark inter-utterance boundary, say one second, and then modify this as further work suggested.

[5]Cf. Hockett 1947, p. 277.

[6]One might perhaps better say: the phonemic structure of the language is that which remains invariant under all possible transformations from one sufficient phonemicization to another.

[7]This definition is a paraphrasing of one by H. Hoenigswald in an unpublished manuscript entitled Meaning.

[8]The procedure is proposed in Harris's forthcoming book Methods in descriptive linguistics.

[9]Meaning, so defined and so used, is it seems to me not avoided in Bloch's postulates (Bloch, 1948).

[10]This, of course, is why we speak of 'reducing' a language to writing

[11]In his paper before the 1948 Christmas meeting of the LSA, M. Joos pointed out that a given operation can occur more than once in a sequence--even in the logical sequence which constitutes one's frame of reference. But this is still something different from the trial-and-error process of actual field work.

EDITORIAL NOTES

With the beginning of volume 7 of SIL, a supplementary series, entitled STUDIES IN LINGUISTICS: OCCASIONAL PAPERS, was inaugurated. No.1, The field of linguistics, by George L. Trager, was distributed with SIL7,1 in March, 1949. Any subscriber who did not receive the pamphlet should write to the Editor for a copy. Additional copies may be secured from the same source for 10¢ each. It is planned to issue further numbers of the OCCASIONAL PAPERS as material comes to hand which by reason of its content or extent calls for distribution different from or supplementary to that of the journal itself; contributions are invited. The page size is 6x9 inches, each number is bound in stiff paper, and arrangments can be made for any desired number of copies, with a sales price appropriate for meeting part or all of the costs of publication; authors may offer subventions, but acceptance for publication will be entirely independent of this.

When volume 5 was completed at the end of 1947, it seemed appropriate to conclude that SIL was a well-established and going concern, and this conclusion was confirmed by a small grant in aid of publication from the American Council of Learned Societies. Accordingly, at the beginning of 1948 invitations to subscribe were again sent to many university libraries, to former subscribers who had discontinued their subscriptions, and to many others. The response, especially from libraries, was encouraging. Almost a fourth of the present subscription list consists of libraries, about half of which began their subscriptions in 1948, most of them purchasing complete back files. A considerable number of exchanges, mostly foreign, was also established. As a result of this expansion, volumes 1 and 2 are out of print, and the supply of volume 3 is very low; any subscribers who wish to dispose of their copies of the first three volumes should communicate with the Editor, as requests for them are still coming in.

The print order for volume 7 has been increased to take care of anticipated new subscriptions. All readers are urged to take out subscriptions or get their institutional libraries to do so. The ACLS has slightly increased its subvention this year, but an increase in individual subscriptions is necessary to obviate the need for raising the subscription price. Any additional funds thus made available will be used for the publication of more articles, or for supplements, or for establishing wider distribution of the journal abroad.

[Pll.'08] [GLT; May 17, 1949]